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Relationship between school dropout and teen pregnancy among rural South African young women

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Abstract

Background: Sexual activity may be less likely to occur during periods of school enrolment because of the structured and supervised environment provided, the education obtained and the safer peer networks encountered while enrolled. We examined whether school enrolment was associated with teen pregnancy in South Africa.

Methods: Using longitudinal demographic surveillance data from the rural Agincourt sub-district, we reconstructed the school enrolment status from 2000 through 2011 for 15 457 young women aged 12–18 years and linked them to the estimated conception date for each pregnancy during this time. We examined the effect of time-varying school enrolment on teen pregnancy using a Cox proportional hazard model, adjusting for: age; calendar year; household socioeconomic status; household size; and gender, educational attainment and employment of household head. A secondary analysis compared the incidence of pregnancy among school enrolees by calendar time: school term vs school holiday.

Results: School enrolment was associated with lower teen pregnancy rates [adjusted hazard ratio (95% confidence interval): 0.57 (0.50, 0.65)].This association was robust to potential misclassification of school enrolment. For those enrolled in school, pregnancy occurred less commonly during school term than during school holidays [incidence rate ratio (95% confidence interval): 0.90 (0.78, 1.04)].

Conclusions: Young women who drop out of school may be at higher risk for teen pregnancy and could likely benefit from receipt of accessible and high quality sexual health services. Preventive interventions designed to keep young women in school or addressing the underlying causes of dropout may also help reduce the incidence of teen pregnancy.

Key words: School enrolment, teen pregnancy, sexual risk, adolescence, South Africa

Key Messages

- Teen pregnancy rates were high in a cohort of young women in rural South Africa with over four pregnancies per 100 person-years.
- School enrolment was associated with lower teen pregnancy rates.
- Estimated conception dates of teen pregnancies were more likely to occur during school holidays than during school terms.

Introduction

Teen pregnancy leads to negative health and social outcomes for both teen mothers and their children.[1–3] Over 16 million births occur to mothers under the age of 20 every year, and nearly all (95%) occur in developing countries.[4] In South Africa, about one-third of young women have borne a child by the age of 20,[5,6] and these births are often accompanied by social stigma and family-imposed sanctions.[7] Identifying protective factors against teen pregnancy, particularly in areas where it is most common, could inform interventions and prevent a large burden of negative health outcomes.

Schooling may be protective against teen pregnancy. At the population level, mass education is a determinant of fertility decline.[8,9] At the individual level, schooling may influence pregnancy risk through sociocognitive development (including increased exposure to sexual health education), through increased human capital or through exposure to different social and sexual networks.[10,11] Additionally, school attendance itself may provide periods of structure and supervision in young adults' lives which could reduce opportunities for sexual activity.[12]

This potential relationship is supported by links between low educational attainment (a measure of highest grade reached by an individual) and teen childbearing,[11,13,14] sexual risk behaviours,[13,15,16] and HIV outcomes.[15,17–19] However, educational attainment provides a summary measure of lifetime education exposure, and it does not establish a temporal relationship between exposure and outcome. Thus, inferences about the directionality of the relationship between educational attainment and sexual risk remain limited.

An alternative education measure explored in this paper is school enrolment status. School dropout is associated with sexual risk behaviours including multiple partnerships, older partner age, unprotected sex and transactional sex,[20,21] and with higher HIV prevalence.[20,22] The relationship between school enrolment and teen pregnancy is more complex. Teen pregnancy among school enrolees leads to subsequent school dropout,[23–26] though, in the South African context, teen pregnancy is not completely incompatible with further schooling.[25] Conversely, school dropout among non-pregnant teens may lead to subsequent pregnancy.[27] Young women who receive incentives to stay in school report fewer pregnancies,[28,29] though the effects of school enrolment cannot easily be isolated from the effects of the incentive itself.

Thus, the relationship between school enrolment and teen pregnancy remains unclear. We intend to contribute to the current literature by exploring this association, motivated by the theorized pathway that school enrolment reduces opportunities for sexual activity. Few studies with longitudinal data have examined the association in the temporal order treating school enrolment as exposure, not outcome. Further, cross-sectional analyses limit the ability to assess directionality of effect.[20–24] Using longitudinal-census data from rural South Africa, in this paper we estimated the association between school enrolment and incident teen pregnancy.

Methods

Study sample

We assembled a cohort of South African young women drawn from the Agincourt sub-district which is covered by a health and socio-demographic surveillance system

(HDSS), located in a rural area of the Bushbuckridge municipality in Mpumalanga Province. The Agincourt HDSS is run by the Medical Research Council/Wits University Rural Public Health and Health Transitions Research Unit. This prospective community study has collected vital events data on all individuals living within the sub-district since 1992. The annual census update collects information on all births, deaths and in- and out-migration, and other individual- and household-level data are collected at less frequent, though regular, intervals.[30,31] Community, household and individual consents have been obtained for all Agincourt HDSS research since its inception. Ethics approval was obtained from the University of the Witwatersrand's Committee for Research on Human Subjects (updated # M110138; original # M960720) and the Mpumalanga Province Health Research and Ethics Committee. Research ethics approval for this analysis was obtained from the Office of Human Research Ethics at the University of North Carolina-Chapel Hill (#13-2013).

We constructed the cohort by identifying all young women between the ages of 12 and 18 years who lived in the study area between 2000–2012. Each young woman began contributing person-time to the study on her 12th birthday. Young women who moved into the study site after their 12th birthday or whose 12th birthday occurred before 1 January 1 2000, were incorporated as late entries. Person-time for each young woman was censored when the first of the following events occurred: (i) administrative censoring date of 31 December 2011; (ii) 18th birthday; (iii) report of educational attainment greater than or equal to 12 years; or (iv) loss to follow-up or death. Young women with recorded pregnancies or high-school graduation prior to the start of follow-up were excluded from this analysis. Young women with multiple records due to internal household movements within the study site were also excluded due to an inability to reliably link them to pregnancy outcomes.

Variables

The outcome in this analysis was incident teen pregnancy. In the yearly census update, fieldworkers administer a pregnancy outcome module to each recently pregnant woman, to collect information on all pregnancies that took place in the previous 12 months. We calculated the estimated conception date for each pregnancy by subtracting 280 days (40 weeks) from the recorded date of delivery. Conception dates for pregnancies recorded as abortions ($n = 9$) were estimated based on the reported duration of pregnancy.

We restricted our outcome to first pregnancy for two reasons: (i) given the relatively young ages in our cohort, we did not expect to see many young women go on to have second pregnancies during follow-up, particularly given that rural South African women tend to delay the timing of their second child;[32,33] and (ii) young women who have previously had a child may be less likely to go on and have further schooling.[23,25]

The exposure in this analysis was school enrolment status. School enrolment was coded as a time-varying, binary exposure equal to one when the participant was enrolled in school and zero when not enrolled in school. An education status module was administered in 1997, 2002, 2006, 2009 and 2012. In the modules, fieldworkers updated the highest education level each young woman had achieved and recorded whether or not she was currently enrolled in school. We recreated a school enrolment history over the follow-up period for each young woman by inferring enrolment status at any given time by examining the change in educational attainment between module years. For example, if a young woman reported grade 7 attainment in 2006 and grade 10 attainment in 2009, we assumed enrolment for 2006, 2007 and 2008. Supplementary Table 1 (available as Supplementary data at *IJE* online) describes our exposure coding decisions for other example data configurations. Observations with illogical education data patterns (e.g. educational attainment decreases over time) were removed from analysis. Observations with non-linear education data patterns indicating grade advancement in any other manner than one grade per year were flagged and incorporated into a sensitivity analysis. Non-linear progression could predominantly be explained by temporary dropout or grade repeat, both of which are common in the rural South African context.[34,35] It is also possible that these observed data patterns were due to unreliable reporting or recording.

We used a directed acyclic graph to identify a minimally sufficient adjustment set of potential confounders of the relationship between school enrolment and teen pregnancy (Supplementary Figure 1, available as Supplementary data at *IJE* online). Specifically, we explored the potential confounding effects of: age, calculated in years from birth date; gender of household head, defined as the gender of the individual reported as the head of household; household head employment status, defined as whether or not the household head reports employment; household head secondary education, defined as whether or not the household head reports at least 12 years of educational attainment; household size, defined as the total number of people reporting membership in the participant's household; household socioeconomic status (SES), measured as a composite index of general SES based on household assets;[36] and calendar year, the year of the participant's 12th birthday, used as an indicator of birth cohort.

For observations with missing covariate information, we used multiple imputation to impute the missing values using the predictive ability of all observed time points of each covariate (six SES observations, 12 household size observations, four household head employment observations, five household head education observations and the single gender of household head observation), as well as the pregnancy outcome. We imputed 30 datasets to reduce the sampling variability using a Markov chain Monte Carlo method. We compared the distribution of covariates before and after imputation to assess comparability.

Statistical analyses

We used Cox proportional hazards models to compare the hazard of first pregnancy among those enrolled in school with those not enrolled in school. The origin for each participant began on her 12th birthday with age as the time scale. We partitioned the dataset so that young women who switched school enrolment status during follow-up could contribute both exposed and unexposed person-time.

As age was the time scale of the Cox model, it was adjusted for implicitly, in unadjusted and adjusted analyses. In all adjusted analyses, we coded originally continuous and ordinal covariates as indicated by log-likelihood tests comparing different functional forms. All time-varying covariates were updated each time new data were provided. Effect measure modification by age was assessed and the results are presented in Supplementary Table 2 (available as Supplementary data at *IJE* online). To assess the sensitivity of our results to uncontrolled household-level confounding, we used a household fixed effects Cox model in which analysis was limited to households with two or more young women from the cohort ($n = 2614$), adjusting for the individual-level covariates of age and calendar year.

We performed sensitivity analyses to assess whether our results were robust to potential exposure misclassification. First, to address the uncertainty around the sequence of events when exposure and outcome occurred around the same time, we removed all person-time contributed by young women with estimated conception dates occurring within 1 year of school dropout. Next, to address the uncertain enrolment status of young women who reported non-linear grade progression between module years, we restricted the sample to remove those flagged as potential grade repeats or grade skips. We also ran the statistical model on a restricted sample combining both of the restrictions mentioned above.

As an additional robustness check, we restricted the sample to those contributing person-time during the data collection window around the census update in 2006 (1 August through 31 December). Under the assumption that school enrolment misclassification was likely to be small immediately following a data update, we compared the risks of pregnancy among those who recently reported enrolment and among those who recently reported non-enrolment, using a log-binomial model.

To further explore whether time spent in school was protective against teen pregnancy, we also investigated the association between school session (summer vacation vs school term) and incident teen pregnancy. To create the time-varying school session variable, we used the exact dates of school holidays between 2000 and 2011 from official South African public school calendars.[37] We partitioned the dataset into person-time contributed to 'school term' (typically mid-January through early December) and 'school vacation'. We then pooled all school term and school vacation person-time and used a Poisson regression model to calculate the incidence rate ratios (IRRs) for teen pregnancy, during school term compared with school vacation. As we assume school calendar time is an exogenous exposure unassociated with individual-level confounders, we calculated unadjusted IRRs. We conducted this analysis in the full sample and stratified by school enrolment status.

Results

Overall, 22 661 young women between the ages of 12 and 18 lived in the study site for at least some period between 1 January 2000 and 31 December 2011 (Figure 1). We removed 512 young women who moved households within the study site during follow-up. A further 136 and 1008 young women were removed who graduated from high school before follow-up began or experienced a pregnancy before follow-up began, respectively. Young women with previous pregnancies typically entered the cohort late, with a mean age of 16.6 years. Also, 1462 and 4086 young women were removed for logically inconsistent education data and missing education data, respectively. Although the number of observations lost due to missing exposure data represented approximately 20% of all eligible young women, only 8% of total person-years were lost. The demographic profile of those excluded due to missing education data was generally similar to those with non-missing education data (Supplementary Table 3, available as Supplementary data at *IJE* online). Our final sample included 15 457 young women.

On average, at the beginning of follow-up, young women were about 13 years old and lived in a household with about seven other people (Table 1). Nearly half of the participants' households (44%) were headed by females and, although over half (59%) of the household heads

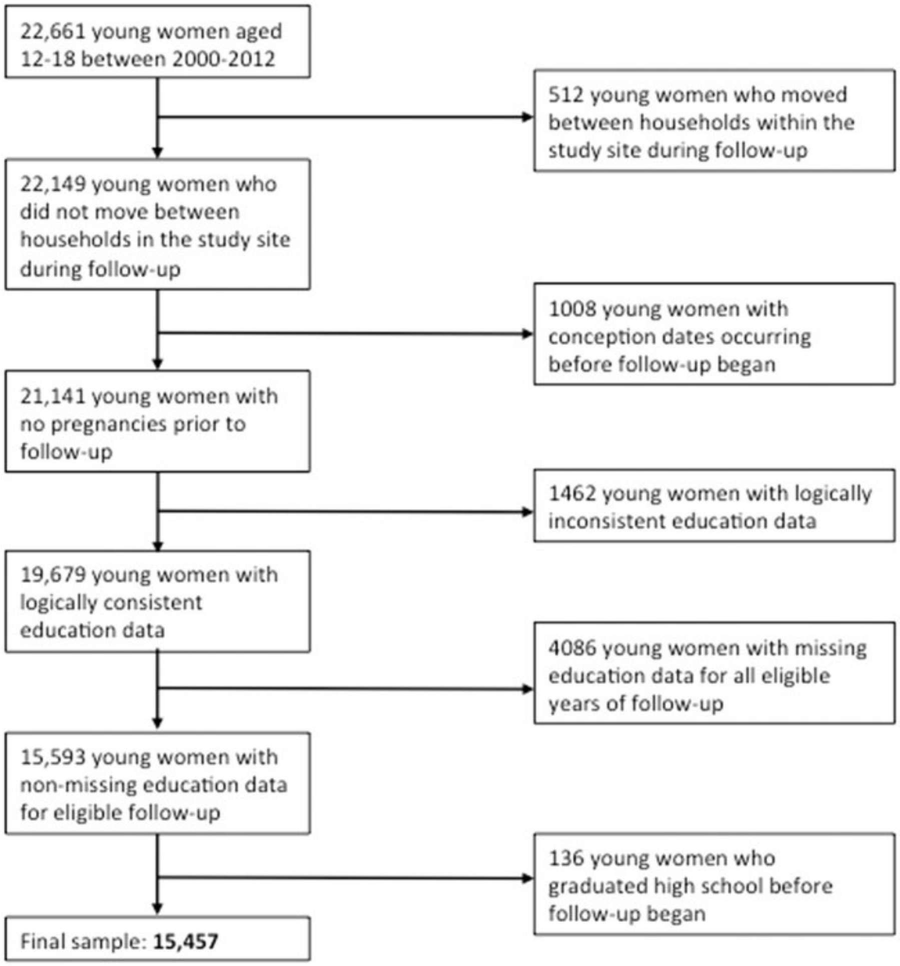

Figure 1. Flowchart of cohort construction of young women, aged 12–18, in Agincourt, South Africa.

Table 1. Baseline covariates of 15 457 young women in Agincourt, South Africa

	Prior to imputation[a]		Post-imputation[b]
Continuous covariates	Mean (SD)	Missing	Mean
Age	13.1 (1.7)	0	13.1
Calendar year at 12th birthday	2003.7 (4.6)	0	2003.7
Household SES	2.3 (0.5)	5495	2.2
Household size	6.8 (3.7)	4291	6.4
Binary covariates	*N* (%)	Missing	%
Female-headed household	6719 (43.9)	146	43.9
Household head employed	7321 (59.0)	3038	58.2
Household head secondary education	1276 (10.6)	3467	9.8

[a]Baseline covariate distribution and missing data patterns as observed in the original dataset

[b]Baseline covariate distribution as observed after multiple imputation used to correct for missing data

were employed, only a small fraction (11%) had graduated from high school. The household SES index was, on average, 2.3 (range: 0.3–3.7); however, baseline SES data were missing for over 35% of all observations. Covariate data for household size, household head employment and household head secondary education were also missing for over 15% of the sample. After using multiple imputation to correct for missing covariate information, the distribution of the covariates was qualitatively similar to those in the non-imputed dataset.

A total of 2140 first pregnancies occurred during the 48 271 person-years contributed (Table 2). The unadjusted pregnancy rate was much lower among school enrolees (4.1 pregnancies/100 person-years) compared with school dropouts (11.7 pregnancies/100 person-years). Accordingly, in the full sample, the hazard of pregnancy was nearly 50% lower among young women enrolled in school compared with young women not enrolled in school. Adjustment for confounding did not substantially affect the association in the complete case analysis or after correction with multiple imputation [multiple

Table 2. Association between school enrolment and teen pregnancy among full and restricted samples of 15 457 young women in Agincourt, South Africa, 2000–12

Cox PH models	Pregnancies	PY	Rate/100 PY	Unadjusted HR (95% CI)	p	Complete case aHR[a] (95% CI)[b]	p	MI aHR[c] (95% CI)[b]	p
Full sample ($n = 15\,457$)									
Not enrolled	261	2238	11.7	–	–	–	–	–	–
Enrolled	1879	46 033	4.1	0.54 (0.48, 0.62)	<0.0001	0.56 (0.48, 0.65)	<0.0001	0.57 (0.50, 0.65)	<0.0001
Restricted sample 1 ($n = 15\,172$): Remove pregnancies within 1 year of dropout									
Not enrolled	159	2181	7.3	–	–	–	–	–	–
Enrolled	1724	45 498	3.8	0.79 (0.67, 0.94)	0.01	0.82 (0.72, 1.02)	0.06	0.84 (0.71, 0.99)	0.03
Restricted sample 2 (n=7776): Linear grade progressors									
Not enrolled	186	1780	10.4	–	–	–	–	–	–
Enrolled	576	17 267	3.3	0.52 (0.44, 0.62)	<0.0001	0.50 (0.40, 0.62)	<0.0001	0.54 (0.46, 0.64)	<0.0001
Restricted sample 3 ($n = 7623$): Restrictions 1 & 2 combined									
Not enrolled	124	1746	7.1	–	–	–	–	–	–
Enrolled	498	17 037	2.9	0.66 (0.54, 0.80)	<0.0001	0.61 (0.47, 0.79)	0.0002	0.70 (0.57, 0.86)	0.0006
Log-binomial model	**Pregnancies**	N	**Risk (%)**	**Unadjusted RR (95% CI)**	p	**Fully adjusted RR (95% CI)[d]**	p		
Restricted sample 4 ($n = 4887$): PT between Aug and Dec 2006									
Not enrolled	2	63	3.2	–	–	–	–		
Enrolled	97	4546	2.1	0.67 (0.17, 2.67)	0.6	0.53 (0.14, 2.11)	0.4		

PY, person-years; PH, proportional hazards; HR, hazard ratio; aHR, adjusted hazard ratio; CI, confidence interval; MI, multiple imputation; RR, risk ratio.

[a]Complete case adjusted analysis restricted to person-time contributed with complete covariate information ($n = 10\,797$)

[b]Adjusted for calendar year of 12th birthday (coded categorically with cutpoints before/after 2006), gender of household head, educational attainment of household head (coded dichotomously at above/below secondary school graduation), employment of household head (coded dichotomously (yes/no) for reported employment at most recent census), household SES (coded linearly), and household size (coded linearly).

[c]Multiple imputation adjusted analysis used full sample with missing covariates imputed using multiply imputed datasets ($n = 15\,457$)

[d]Adjusted for age (coded categorically in one year increments), gender of household head, educational attainment of household head (coded dichotomously at above/below secondary school graduation), employment of household head (coded dichotomously (yes/no) for reported employment at most recent census), household SES (coded linearly), and household size (coded linearly).

imputation-adjusted hazard ratio (MI-aHR) (95% CI): 0.57 (0.50, 0.65)]. Compared with the adjusted estimate from the full sample, results from the household fixed effects model [(aHR) (95% CI): 0.62 (0.14, 2.69)] suggest that the primary findings were not sensitive to uncontrolled household-level confounding. However, this result was measured imprecisely.

These results were reasonably robust to potential misclassification of school enrolment exposure. When observations with pregnancies occurring within 1 year of school dropout were removed, the association between school enrolment and teen pregnancy was still protective, though attenuated [MI-aHR (95% CI): 0.84 (0.71, 0.99)]. When all observations with non-linear grade progression were removed, the results were qualitatively similar to those of the full sample [MI-aHR (95% CI): 0.54 (0.46, 0.64)]. Similar results were also found when the two previous restrictions were combined [MI-aHR (95% CI): 0.70 (0.57, 0.86)]. Our restriction to those contributing person-time during the census update in 2006 also provided effect estimates of similar magnitude [adjusted RR (95% CI): 0.53 (0.14, 2.11)]. However, the estimate was imprecise likely due to the small number of pregnancies during this short time period.

A small association was also observed between school calendar year and teen pregnancy (Table 3). Conception dates for young women were about 10% less likely to occur during the school term compared with during school vacation [IRR (95% CI): 0.90 (0.79, 1.03)] in the Poisson model, though the 95% confidence interval included the null. Interestingly, a similar magnitude of effect was observed for young women enrolled in school [IRR (95% CI): 0.90 (0.78, 1.04)], and not enrolled in school [IRR (95% CI): 0.86 (0.59, 1.25)]. However, the 95% confidence interval for the latter estimate was less precise.

Discussion

We examined the association between school enrolment and incident teen pregnancy in a large, longitudinal cohort of rural, South African young women. We found very high rates of teen pregnancy overall and the hazard of pregnancy was considerably lower during times of school

Table 3. Association between school holidays and teen pregnancy among 15 457 young women in Agincourt, South Africa, 2000–12, by school enrolment status

	Calendar period	Pregnancies	PY	Rate/100 PY	IRR (95% CI)	p
Full sample	School vacation	250	5149	4.9	–	–
	School term	1883	43 128	4.4	0.90 (0.79, 1.03)	0.1
Enrollees	School vacation	220	4924	4.5	–	–
	School term	1653	41 114	4.0	0.90 (0.78, 1.04)	0.1
Non-enrollees	School vacation	30	225	13.3	–	–
	School term	230	2014	11.4	0.86 (0.59, 1.25)	0.4

PY, person-years; IRR, Incidence rate ratio; CI, confidence interval

enrolment than non-enrolment. We also found that pregnancy rates may be lower during school term than during summer holiday. These results together suggest that time spent in school is associated with lower pregnancy rates, consistent with the predictions from several theories,[10] and specifically lending some support to the hypothesis that the structured and supervised environment may contribute to the observed protective association.[12]

The use of prospectively collected census data allowed us to assess the temporal relationship between school enrolment and teen pregnancy. Most previous studies linking school enrolment to sexual risk outcomes had limited ability to make inferences about directionality because they used a cross-sectional design.[20–24] Nonetheless, our observations are consistent with the protective associations observed in these prior studies and in a single longitudinal study using data from the USA, 1988–94.[27](27) Our study extends the findings of a protective effect of school enrolment geographically to South Africa and temporally to the first decade of the 21st century.

School enrolment status was likely measured with some error. Enrolment data were collected with relatively long intervals between collection periods and misclassification may have occurred due to unreliable reporting from interviewees. We performed three analyses to assess the sensitivity of our results to exposure misclassification: (i) removal of all observations with a pregnancy within 1 year of school dropout; (ii) removal of all observations with suspected unreliable reporting (non-linear grade progression); and (iii) restriction to observations contributing person-time during a census update. The findings of these analyses demonstrated that the results were reasonably robust to alternative exposure specifications.

Additionally, we assumed normal pregnancy durations (40 weeks) to calculate conception dates. If pregnancy and school dropout co-occurred with close temporal proximity, misspecification of the pregnancy duration could influence whether the pregnancy was classified as exposed or unexposed. It is plausible that young women who drop out of school are more likely to have preterm births due to lower socioeconomic status and decreased access to care. However, the first sensitivity analysis removed all observations with pregnancies within 1 year of dropout, effectively removing all observations that could have had school enrolment misclassification due to misspecified pregnancy duration. The fact that we still observed a protective, though attenuated, effect in this analysis indicates that pregnancy duration misspecification alone was not a likely explanation for our results.

The year before and year after school dropout may both be high-risk periods for teen pregnancy. The first sensitivity analysis removed all observations with a pregnancy during this potentially high-risk time. The effect estimate moved toward the null, though a protective association was still observed. One explanation for the attenuation of the effect is that some young women may leave school midway through the year due to a pregnancy. This partial year of schooling not recorded in the census could falsely inflate the observed effect estimate in the full sample. Alternatively, the first year after school dropout may be a particularly high-risk time for teen pregnancy and removal of these observations artificially attenuates the effect size in the restricted sample. Although the individual contributions of these two explanations cannot be directly assessed, both are likely to have had some impact.

To more fully understand the relationship between school enrolment and teen pregnancy, results from both randomized and observational studies will need to be compared. Employing a randomized study design to answer this research question is not viable because it is unethical to randomize the widely beneficial school enrolment exposure. Novel study designs that randomize incentives to stay in school could be a good alternative,[28,29,38] but it is difficult to distinguish between the income effects of the incentive and the direct effects of school enrolment. Observational data, as used here, provide an isolated measure of school enrolment status, but the unadjusted results are likely confounded by individual-, household- and community-level covariates. To address some of the drawbacks of analysing observational data, we adjusted for measured covariates to close confounding paths between exposure and outcome. However individual personality traits such as risk proclivity and time preference, as well as household environment factors such as level of parental monitoring, were not measured or controlled for in this analysis. The similarity of the estimates from the fully adjusted model and the household fixed effects model provides some reassurance of the robustness of our primary findings to household environment confounders; however, the possibility that the observed results were influenced by uncontrolled confounding remains.

Finally, pregnancies that did not end in a live birth were likely underrepresented in this analysis. Miscarriages, perinatal deaths and abortions were all likely to be underreported in the census. Abortions, in particular, were reported with a lower frequency than would be expected given the most recent abortion rates in South Africa.[39,40] Unreported abortions could plausibly produce differential outcome misclassification with respect to school enrolment status. However, the magnitude of the observed difference in pregnancy rates between enrolled and non-enrolled young women suggest that the number of unreported abortions among enrolees would have to be improbably high to completely account for the observed effect.

In the secondary analysis, we found that teen pregnancies were more likely to occur during school vacation than during school term. Unexpectedly, this association was observed among both school enrolees and school dropouts. This finding does not necessarily preclude an explanation that school calendar influences teen pregnancy rates. First, the availability of comparably-aged young men (i.e. the ‘supply’ of male partners) is likely to increase during school vacation, which would also increase the risk of pregnancy for young women regardless of their own enrolment status. It is also possible that the festivities and unstructured activities occurring around school holidays could increase the risk of teen pregnancy for all young women, whether they are enrolled in school or not. Other negative behaviors show similar seasonal variation in South Africa, with increased risk corresponding to the school holidays.[41,42] However, it is also possible that the observed association between teen pregnancy rates and school calendar may be explained by other time-varying covariates (e.g. public holidays, cultural festivals, migrant travel) that could lead to seasonality in risk-taking behaviours.

The development of better prevention interventions for South African young women will be critical to reduce the continued high burden of teen pregnancy in this vulnerable population. This study identified school enrolment as a protective factor for teen pregnancy, an outcome with important negative health and social repercussions throughout the life course of the teen mother and child. In South Africa, most young people are enrolled in school for their compulsory school-aged years (ages 7 to 15 years);[43] however, school dropout rates begin to rise after the mandatory enrolment age.[35] Interventions designed to keep young women in school, or to address the underlying reasons for which school dropout occurs, may reduce the burden of teen pregnancy to yield better long-term health outcomes in this population.

Supplementary Data

Supplementary data are available at *IJE* online.

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